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# CURSED BELIEFS WITH COMMON-VALUE PUBLIC GOODS<sup>†</sup>

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**ABSTRACT.** I show how improper conditioning of beliefs can lead to under-contribution in public goods environments with interdependent values. I consider a simple model of a binary, excludable public good. In equilibrium, provision of the public good is good news about its value. Naïve players who condition expectations only on their private information contribute too little, despite the absence of free-riding incentives. In a laboratory experiment, subjects indeed under-contribute relative to equilibrium. Using modified games with different belief conditioning effects, I verify that under-contribution is due to improper belief conditioning. I find little evidence of learning over multiple rounds of play.

**Keywords:** Public goods, experiments, cursed equilibrium, game theory

## I INTRODUCTION

The under-provision of public goods is a central problem in economics. Research on public goods has primarily focused on incentives to free-ride and various mechanisms for overcoming these incentives. In this paper, I demonstrate another force that may drive under-contribution and under-provision, even in the absence of free-riding. In public goods environments with common or interdependent values, individuals may fail to correctly condition their beliefs about the uncertain value of a public good. Many public goods in the real world may have substantial common-value components, such as uncertain quality. Real-world public goods such as pollution abatement, national

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defense, police protection, and flood control may be of uncertain value, and information about the value may be decentralized. Individual contributors to such public goods should condition their beliefs about value on not only their private information, but also the information implicit in the strategic contribution choices of others. Failure to do so may lead to incorrect expectations about the value of the public good.

To isolate the belief conditioning effect of interest, I consider a simple case of a binary, excludable public good (or club good), such as a toll road, private park, or gated community. Consider, for example, the choice of whether to contribute to a new recreation center of uncertain quality. If the use of the center is not tied to contribution, there is an incentive to free-ride. Instead, suppose that contribution takes the form of purchasing a membership, with non-members excluded. In order for the center to be viable, some minimum threshold of contributing members must be reached, otherwise contributions are refunded. Each individual privately observes a signal correlated with the quality or value of the center, and then all individuals simultaneously decide whether to purchase memberships. Any given individual should consider two possible cases: the minimum threshold of members is either reached or it is not. If the threshold is not reached, her decision to contribute is inconsequential, as her money will be refunded. Thus, she should condition her expectations on the event that the threshold is reached. However, this event contains useful information about the quality of the center, since in equilibrium it implies that other contributors observed relatively favorable signals of quality. Thus, an individual who correctly conditions her beliefs on this event should expect the quality to be higher than she would conditional on her private signal alone. Failure to properly condition beliefs would lead to under-contribution and under-provision relative to equilibrium.

Beyond the public goods context, similar effects could arise with joint ventures in which several potential partners have noisy information about the profitability of the venture and must choose whether to participate. Naïve beliefs that fail to account for the information content of others' behavior in these contexts may cause potential partners to under-value the joint venture, thus leading to coordination failure. Thus, the results of this paper may yield

insights into a number of applications within industrial organization as well as public economics.

I develop a simple model of excludable public goods with interdependent values and compare the predictions of Bayesian Nash equilibrium with naïve strategies, formalized by the cursed equilibrium model of Eyster and Rabin (2005). In their model, agents believe that with some probability, others ignore their private information and choose an action according to the (equilibrium) ex ante distribution of actions. Thus, each agent’s belief about the distribution of actions chosen by others is correct, but agents do not fully account for the link between others’ actions and their private information. I show that cursed beliefs lead to under-contribution relative to Bayesian Nash equilibrium, including the possibility of zero contribution for some parameter values.

Testing these predictions in the field would be problematic, since individuals’ private information is unobservable. Therefore, I design a laboratory experiment to test whether improper conditioning of beliefs leads to under-contribution. The main treatment (the common-value threshold game) has 5 players in a group, with a threshold of 4 contributors required for provision. I vary the cost of contribution to determine whether contribution levels conform to Bayesian Nash equilibrium or naïve strategies for high, low, and intermediate costs. Rather than closely mimicking any particular real-world application, the experiment is designed to create a stark separation between the Bayesian Nash equilibrium and (fully) cursed equilibrium predictions to examine the degree to which subjects (fail to) properly condition beliefs in making contribution choices.

Improper belief conditioning has been previously observed in other contexts, most famously in the winner’s curse in common-value auctions. In common-value auctions, bidders should update their belief about value downward conditional on winning, while in my context, contributors should update their belief about value upward conditional on provision. In order to compare the results of the main treatment to the more well-known winner’s curse in common-value auctions, I consider an “anti-threshold” game with the same environment, except that the public good is provided to contributors if and

only if *no more* than 2 players contribute. The anti-threshold game is analogous to a simple common-value, two-unit auction with restricted bids and no trade in the case of excess demand. This treatment allows for comparison of behavioral responses to favorable and unfavorable belief conditioning effects, as well as comparison of how subjects learn to account for these effects over several rounds of play.

Sources of error other than improper conditioning of beliefs might drive behavior away from equilibrium. To isolate the effect of belief conditioning, I consider a treatment with uncertain private values. Each subject has an uncertain private value for the excludable public good and observes a signal correlated with this value. Unlike the common-value case, an individual subject's value is uncorrelated with others players' signals, and thus no subject has information about the value of the public good to others. Play proceeds just as in the main treatment. In this case, the symmetric Bayesian Nash equilibrium strategy precisely corresponds to the naïve (or fully-cursed) strategy from the common-value threshold game. Thus, if subjects are naïve, there should be no difference in behavior between these treatments, while correct conditioning of beliefs should lead to considerably higher contribution in the common-value setting than the uncertain private-values setting.

The experimental results show substantial under-contribution and under-provision in the main treatment. Despite sharp differences in the Bayesian Nash equilibria of the games with favorable, unfavorable, and no belief conditioning effects, actual behavior is quite similar between games, and in fact indistinguishable between the main treatment and the uncertain private values treatment. Furthermore, very little learning is observed. Thus, the results suggest that subjects *completely* fail to condition their beliefs in the proper direction, leading to under-contribution. While fully-cursed equilibrium succeeds in predicting this similarity between treatments, it does not predict contribution levels very accurately.

The paper is organized as follows. Section II explores the related literature. Section III describes the model and theoretical predictions. Section IV details the experimental procedures. Section V shows the results. Section VI

concludes with a discussion of the key findings. Appendices A and B contain proofs and experimental instructions, respectively.

## II RELATED LITERATURE

Many previous experiments consider non-excludable, step-level public goods and provision points, including Van de Kragt et al. (1983), Dawes et al. (1986), Isaac et al. (1989), Marks and Croson (1999), and Croson and Marks (2000). Provision point or threshold mechanisms have been generally successful in such environments under complete information or private values. Several experiments, such as Croson et al. (2006), Kocher et al. (2005), Swope (2002), and Bchir and Willinger (2013) find that excludability tends to increase contributions in a variety of linear and step-level public goods environments, while Czap et al. (2010) find higher contributions to non-excludable projects. Gailmard and Palfrey (2005) compare alternative cost-sharing mechanisms for excludable public goods and find that a voluntary cost-sharing mechanism with proportional rebates performs best.

To my knowledge, the only prior consideration of interdependent-value public goods (excludable or non-excludable) is in the literature on leading by example, beginning with Hermalin (1998), and expanded to charitable giving by Vesterlund (2003), Potters et al. (2005), Andreoni (2006), and Potters et al. (2007). Unlike my symmetric, simultaneous-move setting, this literature examines informed and uninformed players moving sequentially, which is likely to make the information content of the leader's action relatively transparent compared to simultaneous-move games. Indeed, uninformed second-movers do respond to the information contained in the contribution choices of informed first-movers in this environment.

This paper contributes to the public goods literature by showing how naïve beliefs can lead to under-contribution in public goods environments with common or interdependent values, even when free-riding incentives are absent. This effect is conceptually related to the winner's curse in common-value auctions (Thaler 1988, Kagel 1995, Kagel and Levin 2002). In these environments, the bidder with the highest value estimate tends to win the auction,

but because her estimate was the highest, it tends to be higher than the true value. In Bayesian Nash equilibrium, rational agents account for this adverse selection effect and condition their value expectations on winning the auction. However, in many experiments such as Kagel and Levin (1986) and Levin et al. (1996), subjects fail to properly condition beliefs, leading to overbidding and low or negative profits. In my setting, similar naïvety causes subjects to choose not to contribute, even when their signals are high enough that contributing is optimal.

This paper is also closely related to the literature on strategic voting in common-value environments. Seminal theoretical analysis of such environments by Feddersen and Pesendorfer (1996, 1997, 1998) examines the behavior of strategic voters who condition their beliefs on being pivotal. Experiments including Guarnaschelli et al. (2000), Ali et al. (2008), Battaglini et al. (2008), Battaglini et al. (2010), and Esponda and Vespa (2013) find evidence that laboratory subjects sometimes behave strategically, though their behavior is not always explained well by symmetric Bayesian Nash equilibrium.

I am also concerned with comparing behavior and learning under favorable and unfavorable belief conditioning effects. Holt and Sherman (1994) compared these effects in the context of a takeover game. They found evidence of a “loser’s curse” as well as a winner’s curse, with subjects behaving naïvely in both environments.

The concept of naïve behavior in common-value auctions, strategic voting, takeover games, and related environments is formalized by the cursed-equilibrium model of Eyster and Rabin (2005). I will employ Eyster and Rabin’s cursed equilibrium model as an alternative prediction to Bayesian Nash equilibrium and discuss the extent to which this model can explain the experimental data.

### III THEORY

I first give the basic definitions and assumptions. The set of agents is  $N = \{1, \dots, n\}$ , where  $n \geq 2$ . I will use  $i$  and  $j$  to denote typical agents in  $N$ . Each agent observes a private signal  $x_i$ , which is a realization of a random variable  $X_i$ . The private signals are *iid* with probability density function  $f : [\underline{x}, \bar{x}] \rightarrow$

$\mathbb{R}_+$ , which is assumed to be continuous and strictly positive everywhere on the interval  $[\underline{x}, \bar{x}]$ , where  $0 \leq \underline{x} < \bar{x} < \infty$ . Let  $F : [\underline{x}, \bar{x}] \rightarrow [0, 1]$  denote the corresponding cumulative distribution function and  $X$  denote an arbitrary random variable distributed according to  $F$ .

There is a binary excludable public good, and its uncertain value to agent  $i$  is  $v_i$ , given by:

$$v_i = \alpha x_i + \frac{1-\alpha}{n-1} \sum_{j \neq i} x_j, \quad (1)$$

where  $\alpha \in [\frac{1}{n}, 1]$ . The case of  $\alpha = \frac{1}{n}$  corresponds to pure common value, where the value of the public good to all agents is the arithmetic mean of the private signals. The case of  $\alpha = 1$  corresponds to pure private values.

The agents observe their private signals and then simultaneously choose whether or not to contribute an exogenous amount  $w \in (\underline{x}, \bar{x})$  toward provision of the public good. Denote the contribution decision of agent  $i$  given the signal  $x_i$  as  $c_i(x_i)$ , where  $c_i(x_i) = 1$  indicates contribution and  $c_i(x_i) = 0$  indicates non-contribution. The public good is provided if at least  $k \in \{2, \dots, n\}$  agents contribute, otherwise contributions are refunded and no public good is provided. Any agent who does not contribute is excluded and gets a utility of zero. Contributors to the public good get a utility of  $v_i - w$  if the public good is provided, and zero otherwise. All agents are assumed to be risk neutral.

I consider symmetric Bayesian Nash equilibria (BNE), so that in equilibrium,  $c_i \equiv c$  for each agent  $i$ . That is, all agents have identical contribution decision functions. Lemma 1 shows that all such BNE involve “cutoff” strategies.

**Lemma 1.** In any symmetric BNE, there exists  $x^* \in \mathbb{R}$  such that each agent  $i \in N$  strictly prefers to contribute to the public good if and only if  $x_i > x^*$ .

All proofs are contained in Appendix A. Intuitively, Lemma 1 holds because in symmetric BNE, each agent’s expected utility of contributing is non-decreasing in the private signal, and strictly increasing when others contribute with positive probability.



Lemma 2 establishes that, conditional on at least  $k - 1$  others contributing, agent  $i$ 's expectation of the mean signal of the other  $n - 1$  agents is non-decreasing in the cutoff  $x^*$ .

**Lemma 2.** Let the function  $G_i(x^*)$  be given by:

$$G_i(x^*) = E \left[ \frac{1}{n-1} \sum_{j \neq i} X_j \middle| \sum_{j \neq i} c^*(X_j) \geq k-1 \right], \quad (2)$$

where:

$$c^*(X_j) = \begin{cases} 1 & : X_j \geq x^* \\ 0 & : X_j < x^*. \end{cases} \quad (3)$$

Then  $G_i(x^*)$  is non-decreasing in  $x^*$ .

The result in Lemma 2 simply means that the expectation of the mean signal of the agents other than  $i$  conditional on at least  $k - 1$  others contributing is higher than the unconditional expectation, and this conditional expectation is non-decreasing in the cutoff. This result will be useful in proving the first Proposition.

In symmetric BNE, conditional on observing a signal  $x_i = x^*$ , agent  $i$  must be indifferent between contributing and not contributing. Thus,

$$\sum_{l=k-1}^{n-1} \binom{n-1}{l} (1 - F(x^*))^l F(x^*)^{n-1-l} \left( \alpha x^* + \frac{(1-\alpha)l}{n-1} E[X|X \geq x^*] + \frac{(1-\alpha)(n-1-l)}{n-1} E[X|X < x^*] - w \right) = 0. \quad (4)$$

Clearly,  $x^* = \bar{x}$  is a solution, so non-contribution by all agents is a symmetric BNE.<sup>1</sup> Proposition 1 gives conditions for the existence of an interior equilibrium.

**Proposition 1.** There exists a symmetric BNE cutoff  $x^* \in (\underline{x}, \bar{x})$  if and only if:

$$\alpha \underline{x} + (1 - \alpha)E[X] < w < \left( \alpha + \frac{(1 - \alpha)(k - 1)}{n - 1} \right) \bar{x} + \frac{(1 - \alpha)(n - k)}{n - 1} E[X] \quad (5)$$

Moreover, there is at most one such interior symmetric BNE cutoff.

The key to Proposition 1 is to consider agent  $i$ 's expected utility of contributing, given a signal of  $x^*$  and conditional on the public good being provided, treated as a function of the cutoff  $x^*$ . If  $w$  is within the given bounds,

<sup>1</sup>In some cases, this trivial equilibrium may be weakly dominated. If  $w < \alpha \bar{x}$  then agent  $i$  prefers to contribute conditional on observing  $x_i > w/\alpha$ .

this function crosses zero somewhere in the interval  $(\underline{x}, \bar{x})$ . Lemma 2 implies that this function is also strictly increasing in the cutoff, guaranteeing uniqueness of the interior equilibrium cutoff.

Corollary 1 gives comparative static predictions for changes in the cost of contribution and the provision threshold.

**Corollary 1.** Any symmetric BNE cutoff  $x^* \in (\underline{x}, \bar{x})$  is increasing in  $w$  and decreasing in  $k$ .

Intuitively, a higher cost of contribution makes agents less willing to contribute. A higher provision threshold strengthens the favorable belief conditioning effect, increasing willingness to contribute.<sup>2</sup>

### *Cursed Equilibrium*

In (symmetric)  $\chi$ -cursed equilibrium, agents fail to fully account for the connection between the actions of other agents and their private information. Each agent  $i \in N$  believes that with probability  $\chi$ , any given other agent  $j$  contributes with ex ante equilibrium probability regardless of  $j$ 's signal.

Denote the  $\chi$ -cursed equilibrium cutoff by  $x_\chi^*$ . Proposition 2 establishes a simple condition under which a symmetric interior  $\chi$ -cursed equilibrium exists and gives a simple explicit solution for the cutoff in fully-cursed equilibrium, where  $\chi = 1$ .

**Proposition 2.** There exists a symmetric  $\chi$ -cursed equilibrium cutoff  $x_\chi^* \in (\underline{x}, \bar{x})$  if and only if:

$$\alpha \underline{x} + (1 - \alpha)E[X] < w < \left( \alpha + \frac{(1 - \chi)(1 - \alpha)(k - 1)}{n - 1} \right) \bar{x} + \left( \frac{\chi(1 - \alpha)(k - 1)}{n - 1} + \frac{(1 - \alpha)(n - k)}{n - 1} \right) E[X] \quad (6)$$

Moreover, there is at most one such interior symmetric  $\chi$ -cursed equilibrium cutoff. Finally, for  $\chi = 1$ , if there exists an interior symmetric fully-cursed equilibrium cutoff, denoted by  $x_1^*$ , then it is given by:

$$x_1^* = \frac{w}{\alpha} - \frac{1 - \alpha}{\alpha} E[X] \quad (7)$$

<sup>2</sup>This comparative static prediction is not experimentally tested here. However, it guides the experimental design, as choosing  $k$  large relative to  $n$  increases the strength of the favorable belief conditioning effect and thus separation between symmetric BNE cutoffs and cursed cutoffs.

The proof of Proposition 2 is similar to the proof of Proposition 1. Straight-forward manipulation of the expression for the fully-cursed equilibrium cutoff reveals the intuitive interpretation: given a signal equal to the cutoff, the cost of contributing must equal the (naïve) expected benefit (neglecting favorable conditioning).

Corollary 2 establishes that in symmetric cursed equilibrium, agents under-contribute relative to symmetric BNE, and gives comparative statics predictions for the cursed equilibrium cutoff.

**Corollary 2.** The interior symmetric  $\chi$ -cursed equilibrium cutoff  $x_\chi^*$  is non-decreasing in  $\chi$ , increasing in  $w$ , and decreasing in  $k$ . In particular,  $x_\chi^* \in [x^*, x_1^*]$ .

Neglect of the favorable conditioning effect causes agent  $i$ 's expectation of  $v_i$  to be too low, which reduces willingness to contribute. The greater the degree of cursedness ( $\chi$ ), the greater the severity of under-contribution.

Finally, Corollary 3 shows that, for some parameter values, under-contribution in cursed equilibrium may be complete.

**Corollary 3.** If  $\alpha < 1$  and

$$\begin{aligned} & \left( \alpha + \frac{(1-\chi)(1-\alpha)(k-1)}{n-1} \right) \bar{x} + \left( \frac{\chi(1-\alpha)(k-1)}{n-1} + \frac{(1-\alpha)(n-k)}{n-1} \right) E[X] \\ & \leq w < \left( \alpha + \frac{(1-\alpha)(k-1)}{n-1} \right) \bar{x} + \frac{(1-\alpha)(n-k)}{n-1} E[X], \end{aligned} \tag{8}$$

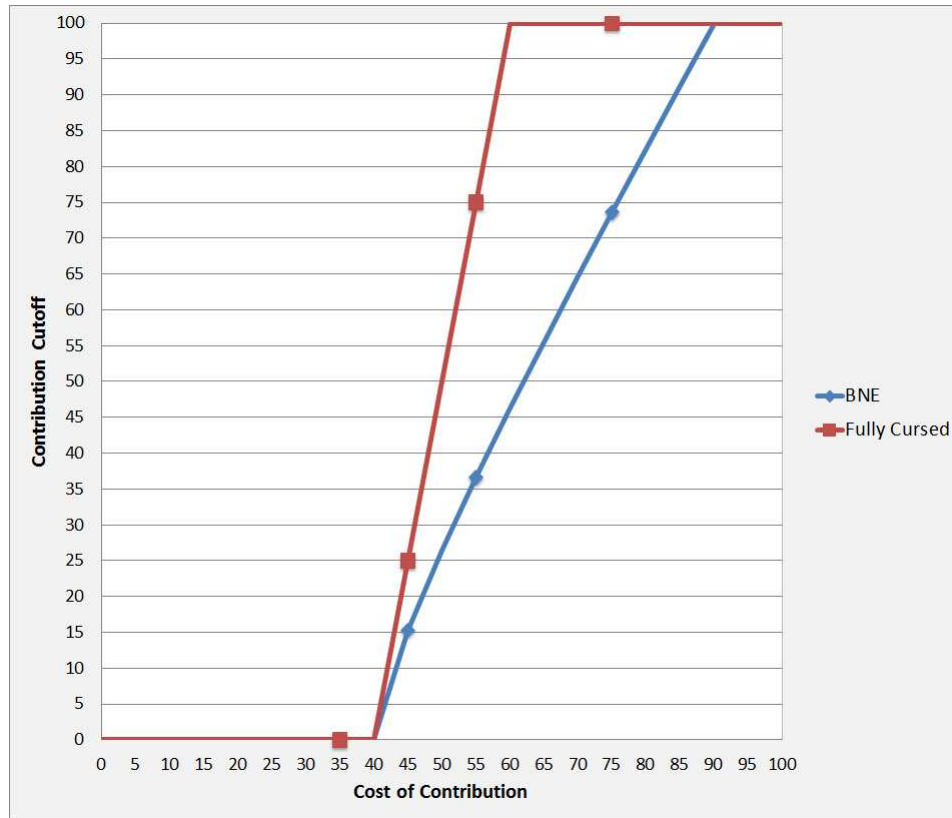
then there is a symmetric BNE such that each agent contributes with positive probability, but in symmetric  $\chi$ -cursed equilibrium contribution occurs with probability zero.

Intuitively, symmetric BNE and fully-cursed equilibrium coincide in the case of pure private values, where other agents' information does not affect agent  $i$ 's expected utility of contributing conditional on the public good being provided. However, when values are interdependent, for some range of  $w$  contribution breaks down completely in  $\chi$ -cursed equilibrium because agents ignore favorable conditioning in forming their expectations.

*Experimental Special Case*

In the common-value threshold game (CVT) treatment, I consider a special case of the threshold game with  $n = 5$ ,  $k = 4$ ,  $\alpha = \frac{1}{5}$ , and private signals that are uniformly distributed on  $[0, 100]$ , with  $w$  varying across rounds of play. Since  $\alpha = \frac{1}{n}$ , this special case is one of pure common value. The pure common value case is used in the experiment because it puts the most weight on the private signals of others and thus gives the greatest contrast between symmetric BNE and fully-cursed equilibrium. Henceforth I will omit the word “symmetric,” since symmetric equilibria are the focus of the paper.

Figure I shows the cutoff signals in BNE and fully-cursed equilibrium for different values of  $w$  in the interval  $[0, 100]$ . The fully-cursed equilibrium cutoff lies (weakly) above the BNE cutoff for all values of  $w$ . That is, the fully-cursed strategy contributes less often than the BNE strategy. Furthermore,



**Figure I.** *Threshold game cutoff signals in BNE and fully-cursed equilibrium*

as in Corollary 3, whenever  $60 \leq w < 90$ , contribution breaks down completely in fully-cursed equilibrium.

It is possible that risk aversion might lead to under-contribution relative to the risk-neutral BNE prediction, and thus it is important to check the robustness of the equilibrium prediction. Allowing for risk aversion makes analytical study of the model much less tractable, but approximate solutions can be found numerically. I use a constant relative risk aversion utility function of the form  $u(y) = \frac{y^{1-r}}{1-r}$  and a coefficient of relative risk aversion of  $r = 0.67$ .<sup>3</sup> BNE cutoffs change very little with risk aversion, rising only by 1-2 percentage points compared to the risk-neutral prediction. Fully-cursed equilibrium cutoffs rise slightly more. Thus, cutoffs exceeding the BNE prediction by magnitudes shown in the fully-cursed equilibrium prediction could not be alternatively explained by plausible risk aversion. Furthermore, the presence of risk aversion does not affect the predicted treatment effects between CVT and the related games of interest.

#### *Anti-Threshold Game with Unfavorable Belief Conditioning*

To compare the favorable conditioning effects in the threshold game to similar unfavorable conditioning effects, I consider an “anti-threshold” (AT) treatment. The environment in the anti-threshold game is the same as in the common-value threshold game, except that the public good is provided if and only if *no more* than  $m$  agents contribute. If more than  $m$  contribute, the public good is not provided and contributions are refunded. The general case of the anti-threshold game is of less interest than the threshold game, so much of the theoretical analysis of the anti-threshold game is omitted. However, the equation characterizing the BNE cutoff is:

$$\sum_{l=0}^{m-1} \binom{n-1}{l} (1-F(x^*))^l F(x^*)^{n-1-l} \left( \alpha x^* + \frac{(1-\alpha)l}{n-1} E[X|X \geq x^*] + \frac{(1-\alpha)(n-1-l)}{n-1} E[X|X < x^*] - w \right) = 0 \quad (9)$$

Notice that  $x^* = 0$  (all agents contributing for all signals) is always a BNE. Under parameter conditions similar to those in the previous section, interior BNE exist as well. The key difference from the threshold game is that in

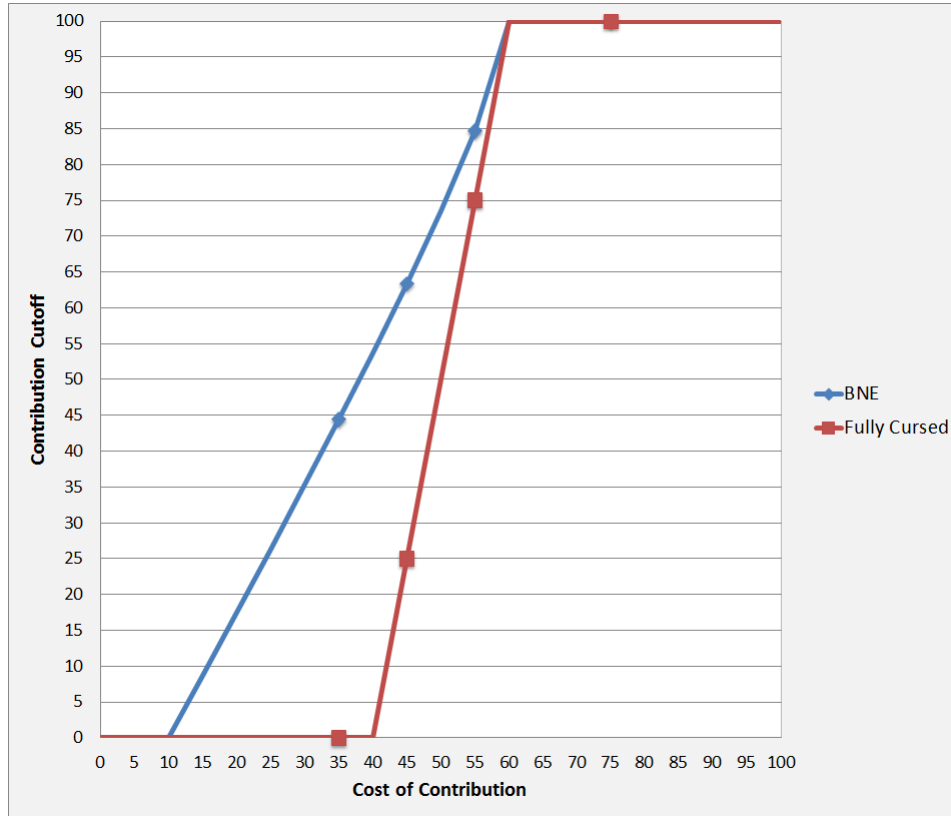
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<sup>3</sup>This level of risk aversion has been found to be typical of laboratory subjects by Holt and Laury (2002).

the anti-threshold game, the public good being provided is *bad news* about its value, while in the threshold game it is good news.

In the AT treatment, I consider the special case of  $n = 5$ ,  $m = 2$ ,  $\alpha = \frac{1}{5}$ , and private signals uniformly distributed on  $[0, 100]$ , with  $w$  varying across rounds of play. Figure II shows the cutoff signals for the anti-threshold game in BNE and fully-cursed equilibrium for varying  $w$ . Notice that cutoffs in fully-cursed equilibrium are exactly the same as those from CVT. However, in AT, fully-cursed agents over-contribute relative to BNE.

There is a simple symmetry between the AT and CVT. Fixing  $\delta \in [-50, 50]$ , the absolute difference between the BNE and fully-cursed equilibrium cutoffs in CVT with  $w = 50 + \delta$  is equal to the absolute difference between BNE and fully-cursed equilibrium cutoffs in the anti-threshold game with  $w = 50 - \delta$ .



**Figure II.** Anti-threshold game cutoff signals in BNE and fully-cursed equilibrium

Thus, the belief conditioning effects in CVT and AT are in this sense comparable in magnitude, but opposite in direction.

*Private-Value Threshold Game with No Belief Conditioning Effect*

In the private-value threshold game (PVT) treatment I consider a game similar to that in CVT, except that each agent’s value for the public good is the mean of five agent-specific *iid* random draws. One of the five is observed by the agent, while the other four are not observed by anyone. Thus, the ex ante marginal distribution of each agent’s value is the same as in CVT, but there is no conditioning effect. In fact, the symmetric fully-cursed equilibrium in CVT is identical to the symmetric Bayesian Nash equilibrium in PVT. Thus, by comparing contributions between PVT and CVT, the effect of favorable belief conditioning CVT can be observed.

#### IV EXPERIMENTAL PROCEDURES

To avoid negative payoffs, the cost of contributing is implicit, so that each participant is faced with a choice between a certain payoff of  $w$  and an uncertain payoff of  $v$ .<sup>4</sup> The conversion rate is \$0.20 for each experimental currency unit (or “token”), so that the maximum possible earnings are \$20 per person. Participants also received a \$5 show-up fee. Subjects gave consent to access academic records including Grade Point Average, ACT/SAT scores, and academic major. This information is used to test whether behavior in the experiment is correlated with cognitive or quantitative ability.

There are two treatment variables. The first, varied between subjects, is the game: common-value threshold (CVT), anti-threshold (AT), and private-value threshold (PVT). Only one of the three games appeared in any given session. The second treatment variable, varied within subject, is the cost of contributing: 35, 45, 55, and 65 experimental currency units, with each value repeated five times in randomized order. Each session had twenty rounds, one

<sup>4</sup>Framing in terms of explicit rather than implicit costs might affect behavior and learning (Lind and Plott, 1991). However, in the treatment of primary interest (CVT), the experience from which subjects are expected to learn not to under-contribute is the failure to realize profitable public goods, which is inherently implicit.

of which was selected randomly for payment. Each session included twenty participants who were randomly assigned to groups of five at the start of each round (stranger matching).

In each round, each participant observed the cost of contributing and her own private signal. Contribution choices were then made simultaneously. At the end of each round, all participants observed the signals and choices of the other four group members (ordered from highest to lowest), the value of the public good and whether it was provided, and their own earnings in tokens for the round.<sup>5</sup>

The experiment was programmed and conducted using z-Tree software (Fischbacher, 2007). All sessions were run in the experimental economics laboratory at The Ohio State University. Seven sessions were run (3 CVT, 2 AT, and 2 PVT), each with 20 subjects.<sup>6</sup> Participants earned approximately \$15.50 on average, and each session lasted about 45 minutes.

## V RESULTS

To organize the results, I first summarize the key hypotheses to be tested. The main hypotheses come from the predictions of cursed equilibrium compared to Bayesian Nash equilibrium.

**Hypothesis 1** (Contribution within Games). Under full or partial cursedness, subjects will choose to allocate tokens to the group project too little in CVT and too much in AT, relative to BNE.

**Hypothesis 2** (Contribution between Games). Under full cursedness, subjects will choose to allocate tokens to the group project with the same frequency in CVT, PVT, and AT.

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<sup>5</sup>The signals and choices of other group members were displayed in decreasing order by signal to make it easier to notice any correlation between signals and choices.

<sup>6</sup>Due to a recruitment system error, two subjects were mistakenly allowed to participate a second time. The choices made by each of these subjects in their second session of participation have been excluded from the analysis.



Hypothesis 1 comes from the neglect of belief conditioning in CVT and AT under full or partial cursedness. Hypothesis 2 comes from the fact that the fully-cursed equilibrium cutoffs in CVT, PVT, and AT are identical.<sup>7</sup>

Secondary hypotheses of interest are concerned with learning over multiple rounds of play and individual heterogeneity. I will investigate whether subjects learn to play closer to BNE and whether learning effects differ between games. I will also examine whether individuals with greater cognitive or quantitative ability play strategies closer to BNE.

**Hypothesis 3** (Learning to Play BNE). If subjects learn to recognize favorable and unfavorable belief conditioning effects, their allocation decisions should move toward BNE after several rounds of play in both CVT and AT.

**Hypothesis 4** (Individual Heterogeneity and Cognitive/Quantitative Ability). Greater cognitive and quantitative ability will be positively correlated with proper belief conditioning.

### *Aggregate Results*

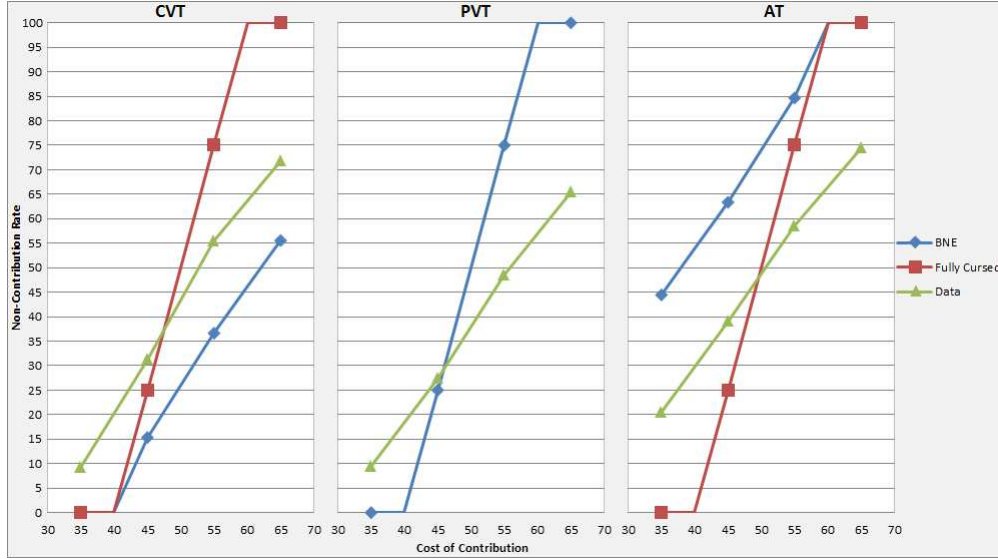
Aggregate rates of allocation to the private account are summarized in Figure III. It is evident that aggregate under-contribution occurs in CVT, though not to the degree predicted by fully-cursed equilibrium for the higher cost levels where the difference is greatest. It is also clear that subjects choose the group project too frequently in AT relative to BNE, though the frequencies are not close to the fully-cursed equilibrium predictions either.

Notice that contribution rates in CVT and PVT are virtually the same for all cost levels.<sup>8</sup> I find no statistical difference between CVT and PVT for any cost level using Wilcoxon-Mann-Whitney tests with subject-level average contribution as the unit of observation. Contribution rates in AT are somewhat lower, though the difference is much less stark than predicted under BNE.

Overall, the similarity of contribution rates across games is consistent with full or nearly-full cursedness. However, the contribution rates within each

<sup>7</sup>There is no distinction between cursed equilibrium and BNE in PVT, due to the absence of belief conditioning effects.

<sup>8</sup>Even where the difference is greatest (the higher cost levels) it is in the opposite direction predicted by BNE, with slightly less contribution in CVT than in PVT.

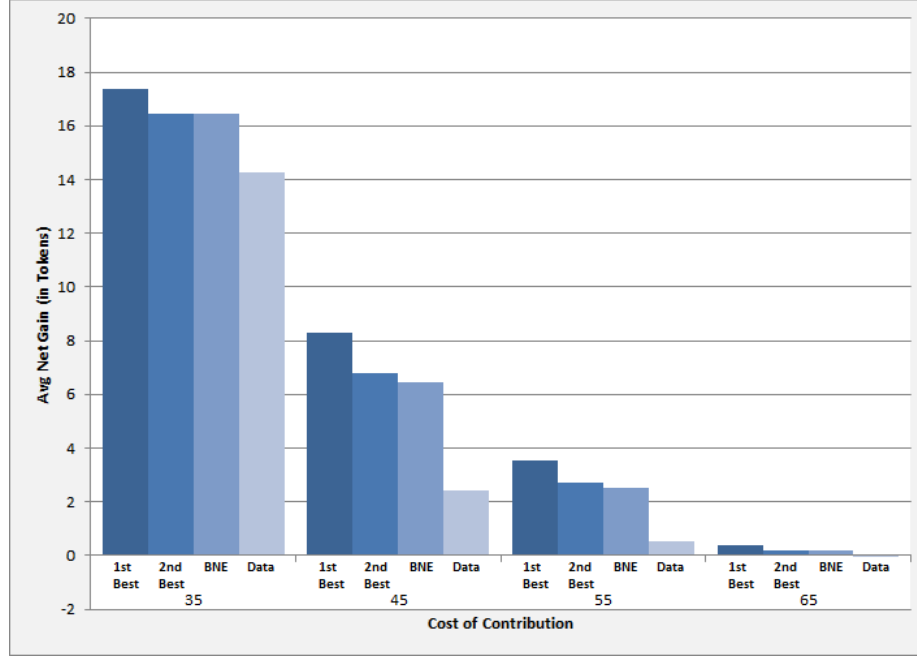


**Figure III.** Aggregate rates of non-contribution (choosing the private account)

game are not particularly close to the fully-cursed predictions. Moreover, it is clear that cursedness does not explain the data completely, because contribution levels in PVT differ substantially from the (identical) predictions of BNE and cursed equilibrium. This result highlights the importance of studying belief conditioning by comparing the CVT and PVT treatments rather than only comparing the data to theoretical benchmarks within one treatment. Later, I will explore possible explanations for the contribution levels in PVT.

In addition to contribution decisions, efficiency in CVT is of interest. Figure IV shows the average per person net gains in CVT for each cost of contribution. The first-best efficiency benchmark shows the net gain if provision occurs if and only if provision is efficient. The second-best benchmark shows the net gain if a benevolent social planner were to enforce a symmetric contribution cutoff to maximize the expected total surplus. While efficiency under BNE is somewhat lower than second-best, it is quite close. However, the efficiency in the data falls well below even the BNE benchmark, particularly for cost levels 45 and 55. Overall, there is a loss of approximately 65% of the average net gains from the public good that could have been realized in BNE.

While the rates of choosing the private account may provide a rough estimate of the average cutoff subjects use, a more appealing method is maximum

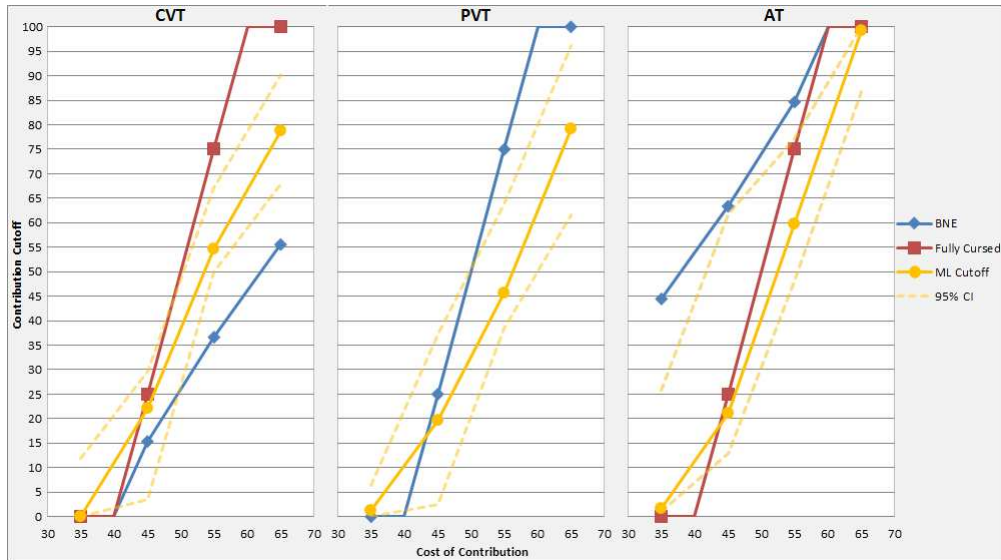


**Figure IV.** *Efficiency in CVT*

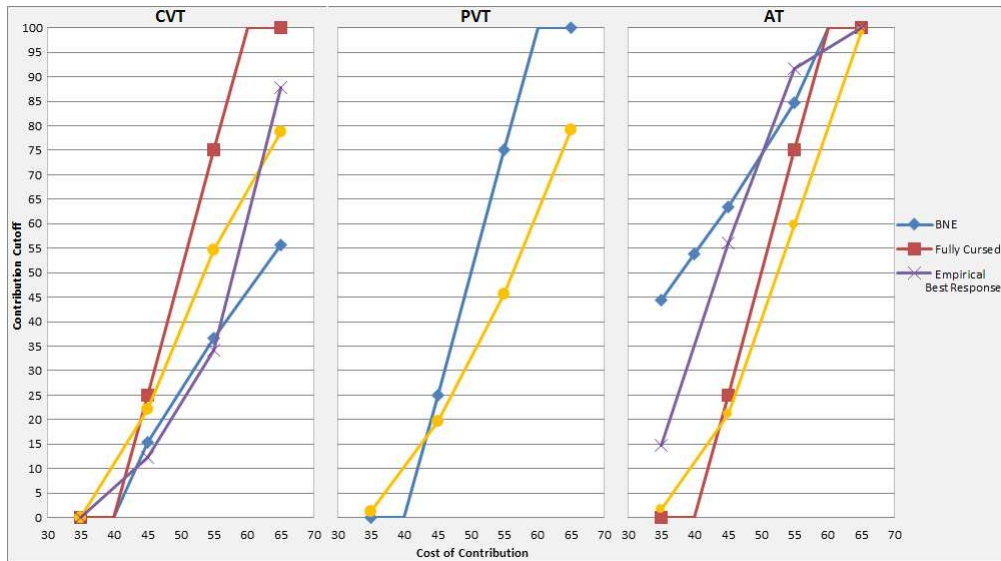
likelihood, similar to the method of El-Gamal and Grether (1995). Under the assumption that all subjects use the same cutoff (but may make errors), I estimate the cutoff for each game and cost level by checking all possible cutoffs and finding the one that explains the most data, or equivalently, minimizes the number of errors. I assume that with probability  $1 - \epsilon$ , an agent makes a contribution choice consistent with the hypothesized cutoff, and with probability  $\epsilon$  (the error rate), she makes the opposite choice. The maximum likelihood cutoff is the cutoff that minimizes the observed error rate.

Figure V shows maximum-likelihood cutoffs with 95% bootstrap confidence intervals. Once again, the estimated cutoffs suggest under-contribution in CVT and over-contribution in AT relative to BNE. It is also clear that estimated cutoffs are very similar between CVT and PVT. I find no significant differences between the CVT and PVT cutoffs for any cost level using bootstrap hypothesis tests.<sup>9</sup> This similarity further suggests that subjects treat

<sup>9</sup>These tests are not meant to be interpreted as independent of the previous tests comparing contribution rates, but together to they give a clearer description of the similarity between the CVT and PVT data.



**Figure V.** *Maximum Likelihood Cutoffs*



**Figure VI.** *Empirical Best Responses*

the CVT and PVT games as equivalent, despite the substantial differences between their BNE.

While behavior does not appear to be consistent with BNE, it is also of interest how closely behavior approximates an empirical best response. Figure VI compares maximum likelihood cutoffs with empirical best response

cutoffs. Empirical best response cutoffs can be easily computed from equation 1 using the empirical probability of contribution to the group project, average signal conditional on contribution, and average signal conditional on non-contribution. Maximum likelihood cutoffs are generally not very close to empirical best response cutoffs where the cutoffs are interior, with the exception of CVT with  $w = 65$ .<sup>10</sup> Thus, neither BNE, nor fully-cursed equilibrium, nor empirical best response appears to explain the aggregate data well.

Assuming some partially-cursed equilibrium holds across all rounds and all cost levels, the cursedness parameter  $\chi$  can be estimated by maximum likelihood for CVT and AT.<sup>11</sup> For CVT, the maximum likelihood estimate of the cursedness parameter is 0.60, with a 95% bootstrap confidence interval of [0.58, 0.76]. In fact, the 0.6-cursed equilibrium cutoffs are quite close to the previous (unrestricted) maximum likelihood cutoffs estimates for CVT.

For AT, the maximum likelihood estimate of the cursedness parameter is 0.91, higher than in CVT, but with a wide 95% bootstrap confidence interval of [0.51, 1.00]. Furthermore, the 0.91-cursed equilibrium cutoffs are not particularly close to the unrestricted maximum likelihood cutoff estimates, since for cost levels 45 and 55 the unrestricted estimates do not fall between the BNE and fully-cursed equilibrium cutoffs. The data in AT are somewhat noisier than in the other games, perhaps due to the less intuitive nature of the game.

The following main results summarize the key findings from the aggregate data.

**Result 1** (Contribution within Games). Relative to BNE, subjects allocate tokens to the group project too infrequently in CVT and too frequently in AT. Subjects also over-contribute in PVT with cost levels of 55 and 65, which is not predicted by cursedness.

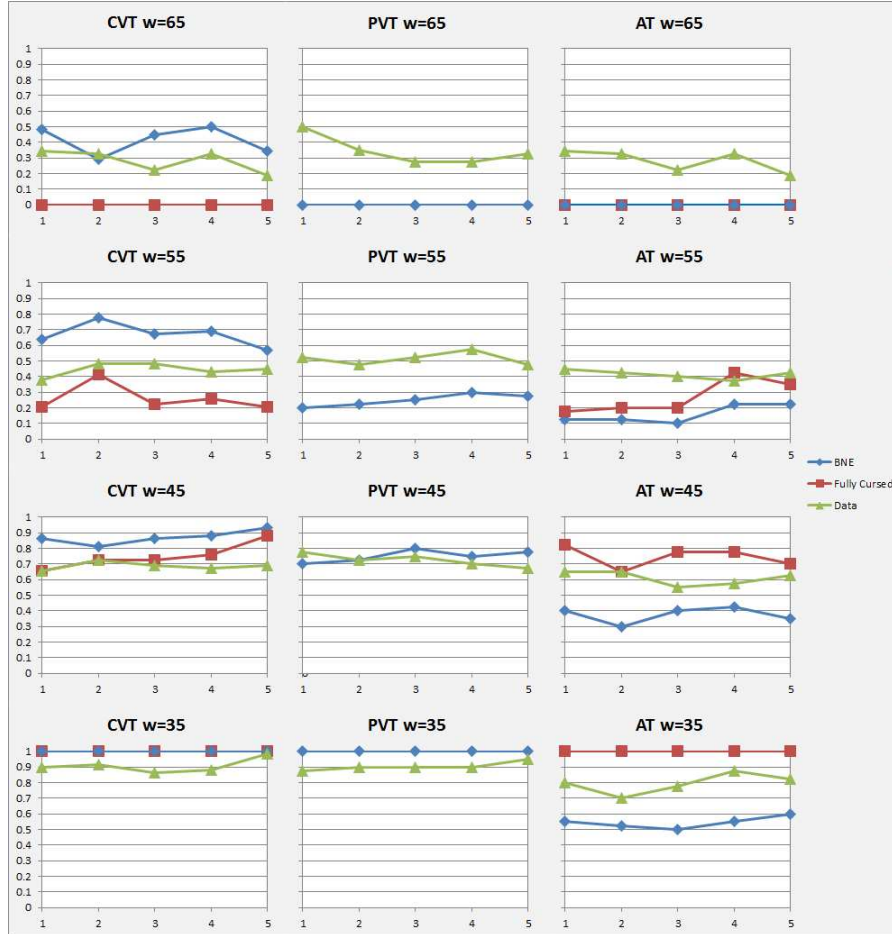
<sup>10</sup>Empirical best response cutoffs may be either higher or lower than BNE cutoffs, depending on behavior. Using a cutoff above the BNE cutoff tends to drive the empirical best response cutoff downward in CVT, since the favorable conditioning effect is strengthened. However, the opposite type of “mistake” (contributing when the signal is too low) has the opposite effect on the empirical best response cutoff.

<sup>11</sup>This estimation follows the same approach of selecting cutoffs to minimize errors as previously discussed. However, I add the restriction that cutoffs for each of the four cost levels within a game (CVT or AT) must be consistent with some partially-cursed equilibrium cutoff.

**Result 2** (Contribution between Games). Contribution choices in CVT and PVT are indistinguishable. Contribution choices in AT differ from those in the other games, but this difference is much smaller than predicted in BNE.

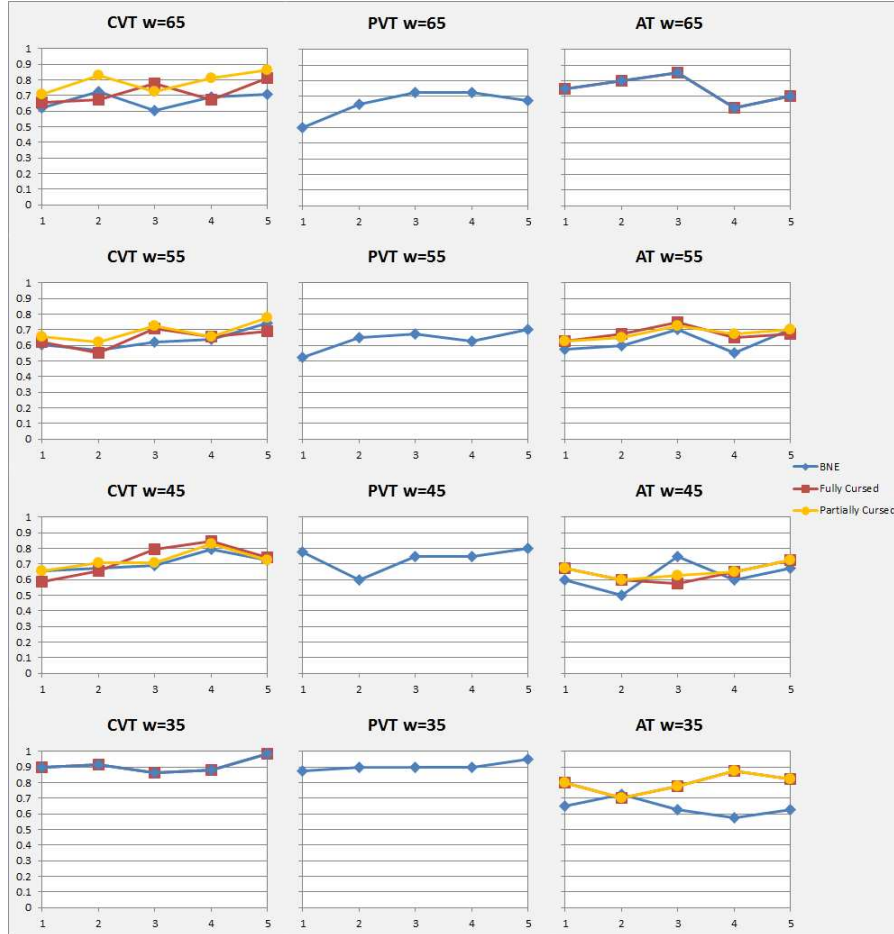
### *Repeated Trials and Learning*

Before interpreting the aggregate results, I examine behavior and learning over multiple rounds of play graphically and using logistic regression analysis. Recall that each of the four cost levels was encountered five times in each session, with the order randomized for each session.



**Figure VII.** *Contribution Rates Over Repeated Trials*

Figure VII shows contribution rates over repeated trials of each of the four cost levels and for each of CVT, PVT, and AT. The BNE and fully-cursed benchmarks represent the expected contribution rates under each equilibrium concept, given the signals realized in the experiment. Few clear trends are apparent. Contributions do appear to decline in PVT with  $w = 65$ , which is somewhat reassuring given that no contribution should occur in that case. Only in CVT and PVT with  $w = 35$  (where everyone should always contribute) does behavior seem to approach BNE. Overall, only in these few simple cases, where subjects should always or never contribute, do the data seem to suggest learning patterns.



**Figure VIII.** *Equilibrium Match Over Repeated Trials*

	<b>w=65</b>		<b>w=55</b>		<b>w=45</b>		<b>w=35</b>	
<b>Variable</b>	OR	<i>p</i> -value	OR	<i>p</i> -value	OR	<i>p</i> -value	OR	<i>p</i> -value
signal	1.026	0.002***	1.033	0.000***	1.028	0.013**	1.008	0.407
period	0.960	0.124	0.988	0.712	0.950	0.135	1.036	0.418
CVT	0.408	0.300	0.397	0.238	0.546	0.310	0.763	0.732
AT	0.709	0.717	0.667	0.588	0.405	0.148	0.180	0.019**
signal*CVT	1.007	0.528	1.001	0.941	1.000	0.978	1.004	0.781
signal*AT	0.981	0.096*	1.002	0.857	0.998	0.891	1.016	0.260
period*CVT	1.021	0.564	1.046	0.285	1.047	0.253	1.009	0.854
period*AT	1.094	0.102	0.975	0.581	1.043	0.330	1.001	0.984

<b>Linear Combn</b>	OR	<i>p</i> -value	OR	<i>p</i> -value	OR	<i>p</i> -value	OR	<i>p</i> -value
signal*(1+CVT)	1.033	0.000***	1.033	0.000***	1.027	0.003***	1.012	0.299
signal*(1+AT)	1.007	0.389	1.034	0.000***	1.026	0.000***	1.024	0.016**
period*(1+CVT)	0.980	0.413	1.033	0.197	0.995	0.797	1.046	0.106
period*(1+AT)	1.050	0.310	0.963	0.237	0.991	0.738	1.037	0.171
AT-CVT	1.737	0.580	1.679	0.434	0.741	0.537	0.236	0.073*
signal*(AT-CVT)	0.974	0.018**	1.001	0.896	0.999	0.878	1.011	0.463
period*(AT-CVT)	1.071	0.202	0.932	0.084*	0.996	0.915	0.992	0.824

**Table I.** Logistic regression results. The dependent variable is an indicator for contribution. Robust standard errors are clustered by individual subject. Each regression has 690 observations with 138 subject-level clusters. \*\*\*, \*\*, and \* indicate significance at the 1%, 5%, and 10% levels, respectively.

Figure VIII shows the proportion of choices consistent with several equilibrium concepts over repeated trials. The particular partially-cursed equilibrium used here is for the maximum likelihood values of  $\chi$  for CVT and AT (0.6 and 0.91 respectively).<sup>12</sup> Again, there is little evidence of significant learning or convergence toward BNE, except in the simpler cases where subjects should always or never contribute. The clearest differences in consistency of contribution choices with the equilibrium concepts are in the cases of greatest contrast between BNE and cursed cutoffs (CVT with  $w = 65$  and AT with  $w = 35$ ). However, less differentiation is apparent for cost levels where there is less contrast between cutoffs under each equilibrium concept.<sup>13</sup>

<sup>12</sup>Likelihood ratio tests confirm that the MLE partially-cursed equilibria fit the data significantly better than BNE with  $p$ -value  $< 0.001$  for both CVT and AT.

<sup>13</sup>Relatively few observations in these cases fall in the range where the equilibrium concepts make different predictions.



Table I shows logistic regression results, where the dependent variable is an indicator for contribution to the group project.<sup>14</sup> The omitted category for the game indicators is PVT. The second panel of Table I shows linear combinations of estimated effects, including the effect of signal and period in CVT and AT, as well as differences between these treatments and effects. First, it is clear that subjects do respond to signals when they should. Except in CVT and PVT with  $w = 35$  and AT with  $w = 65$  (where either contribution or non-contribution is always optimal), the signal effect is strongly significant. Notice also that the effect of period of play is not significant for any game or cost level. The one possible exception is the case of CVT with  $w = 35$ , where the  $p$ -value approaches the 10% level. Finally, notice that the CVT indicator and its interactions with signal and period are insignificant for all cost levels, consistent with the previous finding from the aggregate results that behavior in CVT and PVT is indistinguishable, despite stark differences in their BNE strategies.

Overall, behavior over multiple periods of play shows little evidence of learning, and confirms the strong similarity between strategies in CVT and PVT. Result 3 summarizes the findings on repeated trials and learning.

**Result 3** (Learning to play BNE). Subjects do not appear to learn to play strategies closer to BNE, except possibly in some simpler cases where belief conditioning effects are absent and equilibrium cutoffs are 0 or 100. Behavior in CVT and PVT remains very similar, even after several periods of play.

### *Individual Heterogeneity*

Individual subjects in CVT and AT may differ in their ability to properly condition beliefs. I test whether cognitive and quantitative ability is correlated with proper belief conditioning in CVT and AT (Hypothesis 4). The logistic regression results in Tables II and III include as explanatory variables individual subjects' college Grade Point Average (GPA) and an indicator for majoring

<sup>14</sup>I report the logistic regression results in terms of odds ratios to simplify the interpretation of interactions as multiplicative effects (Buis, 2010). However, computation of marginal interaction effects using the method of Ai and Norton (2003) does not lead to substantively different results.

	<b>w=65</b>		<b>w=55</b>		<b>w=45</b>		<b>w=35</b>	
<b>Variable</b>	OR	<i>p</i> -value	OR	<i>p</i> -value	OR	<i>p</i> -value	OR	<i>p</i> -value
signal	1.034	0.001***	1.032	0.000***	1.025	0.000***	1.005	0.744
period	0.974	0.397	1.014	0.733	0.985	0.562	1.004	0.885
GPA	0.405	0.151	0.867	0.740	1.462	0.223	3.125	0.072*
quant	0.478	0.111	0.656	0.335	1.070	0.824	1.318	0.634

**Table II.** Logistic regression results for CVT including GPA and quantitative major indicator. The dependent variable is an indicator for contribution. Robust standard errors are clustered by individual subject. Each regression has 200 observations with 40 subject-level clusters. \*\*\*, \*\*, and \* indicate significance at the 1%, 5%, and 10% levels, respectively.

	<b>w=65</b>		<b>w=55</b>		<b>w=45</b>		<b>w=35</b>	
<b>Variable</b>	OR	<i>p</i> -value	OR	<i>p</i> -value	OR	<i>p</i> -value	OR	<i>p</i> -value
signal	1.007	0.360	1.035	0.000***	1.026	0.000***	1.025	0.016**
period	1.047	0.339	0.962	0.235	0.991	0.748	1.038	0.171
GPA	0.939	0.895	0.624	0.142	0.946	0.892	1.971	0.182
quant	0.420	0.058*	0.658	0.245	0.813	0.606	1.233	0.679

**Table III.** Logistic regression results for AT including GPA and quantitative major indicator. The dependent variable is an indicator for contribution. Robust standard errors are clustered by individual subject. Each regression has 200 observations with 40 subject-level clusters. \*\*\*, \*\*, and \* indicate significance at the 1%, 5%, and 10% levels, respectively.

in a quantitative field. The majors I classify as quantitative are mathematics, statistics, engineering, natural and physical sciences, computer science, economics, finance, and accounting. The results in Table II show some evidence of a positive correlation between GPA and contribution at cost level 35, but no other such correlations are apparent. Table III shows similar logistic regression results for AT. Quantitative majors contribute less in AT for cost level 65, but otherwise the results are again quite negative.<sup>15</sup> Similar to the results on learning over multiple rounds of play, the only apparent correlations here are in treatments where the equilibrium strategy is simply to always contribute or never contribute.

<sup>15</sup>The general negativity of these results is robust to alternative specifications including additional controls and interactions, as well as substituting ACT/SAT percentile in place of GPA. While GPA is not available for all subjects, reducing the sample size somewhat, it is available for more subjects than ACT/SAT scores.

I next specify several candidate strategies to which individual choices are compared. The candidate strategies include BNE and fully-cursed equilibrium, as well as several other possible heuristics: contributing iff  $w < 50$  (“Prior”), contributing iff the signal exceeds  $w$  (“Signal Bias”), always contributing, and never contributing.

I use a Bayesian approach to estimate the proportion of subjects in each candidate strategy. I assume that each individual is playing one of the candidate strategies, but may make errors. In any individual game I assume that with probability  $1 - \epsilon_i$ , player  $i$  follows her chosen strategy, and with probability  $\epsilon_i$  she deviates. First, an individual subject’s choices over all twenty rounds are compared to the predictions of each candidate strategy. The error rate  $\epsilon_i$  for player  $i$  is estimated as her smallest observed frequency of deviations over all candidate strategies.<sup>16</sup> So for example, if player  $i$ ’s choices were consistent with the fully-cursed strategy 95% of the time and less frequently consistent with any other strategy, her estimated error rate would be 0.05. Next, I set a uniform prior over all candidate strategies and update for each observation according to Bayes’ rule to arrive at a posterior over the candidate strategies for each individual subject. For each candidate strategy, the posterior probability is averaged across subjects to estimate the overall proportion of subjects playing that strategy.<sup>17</sup>

Table IV shows the estimated proportion of subjects playing each candidate strategy in CVT and AT. In both games, the fully-cursed strategy is modal, consistent with the aggregate results showing neglect of belief conditioning. The BNE strategy is second most prevalent in CVT with a proportion of nearly one quarter, though the vast majority appear to play some boundedly-rational

<sup>16</sup>There are very few subjects who are always consistent with a single candidate strategy, but 21.7% of subjects in CVT are at least 95% consistent. Among these subjects, just over half closely match the fully-cursed strategy, while about one quarter closely match BNE. Only 7.5% of subjects in AT are at least 95% consistent.

<sup>17</sup>The results are reasonably robust to alternative error structures and non-uniform priors. The MLE partially-cursed strategy is not included as a candidate strategy since it is a free parameter estimated from the data rather than being specified a priori. However, if it is included, it becomes modal in CVT and second most prevalent after the fully-cursed strategy in AT, and the prevalence of BNE falls substantially.

Strategy	CVT		AT	
	Proportion	Std. Error	Proportion	Std. Error
Fully-Cursed	0.312	0.047	0.359	0.054
BNE	0.233	0.046	0.130	0.035
Prior	0.219	0.043	0.154	0.035
Signal Bias	0.120	0.034	0.187	0.044
Always	0.108	0.036	0.097	0.036
Never	0.008	0.003	0.073	0.036

**Table IV.** *Estimated strategy proportions in CVT and AT*

strategy. The BNE strategy is less prevalent in AT than CVT, which might suggest that AT is a more difficult game.

To check for correlations between consistency with the BNE strategy from the type estimation and cognitive/quantitative ability, I have run a number of regressions similar to those in Tables II and III. However, the results have been similarly negative, suggesting that some subjects' behavior may simply appear to closely match BNE by chance rather than strategic sophistication.<sup>18</sup> Furthermore, estimating strategy proportions in the PVT data using the CVT strategies also yields an estimate of approximately one quarter of subjects playing the BNE for the CVT game. However, the BNE strategy from CVT does not have any particular justification or heuristic intuition in the PVT game. Recall that the actual BNE strategy in PVT is identical to the fully-cursed equilibrium strategy from CVT. Thus, there is no reason to expect subjects in PVT to play the BNE strategy from CVT, except perhaps by chance. The similarity of estimated proportions in CVT and PVT playing the BNE strategy from CVT further suggests that strategic sophistication does not drive consistency with the BNE strategy in CVT. Therefore, it appears that very few if any subjects are able to properly condition beliefs in this setting.

**Result 4** (Individual Heterogeneity and Cognitive/Quantitative Ability). There is little evidence that behavior in CVT or AT is correlated with cognitive or quantitative ability, except possibly in some simpler cases where conditioning

<sup>18</sup>This finding is similar to Georganas et al. (2012), who found very little correlation between measures of cognitive ability and playing more sophisticated strategies in undercutting and guessing games.

effects are absent and the equilibrium cutoffs are 0 or 100. Estimated proportions of strategic types show that fully-cursed behavior is modal, and that the great majority of subjects play some boundedly-rational or heuristic strategy.

### *Over-Contribution in PVT*

As shown in the previous results, there is a puzzling tendency for subjects to over-contribute in PVT (and AT) with  $w = 65$ . Even with a signal of 100, the expected value of the group project is no greater than 60 tokens, and thus subjects should never contribute in PVT when the cost is 65 tokens. Interestingly, subjects do not frequently make similar mistakes in PVT with  $w = 35$ , where contribution is always optimal for any signal. Thus, there is an asymmetry in behavior between cases in which subjects should always or never contribute.

This finding is not driven by a small subset of subjects. Rather, a majority of subjects contributed at least once in this case. This behavior is clearly not driven by cursedness, because in PVT full or partial cursedness yields the same prediction as BNE. Rationalizing this behavior through risk preferences would require many subjects to be implausibly risk-loving, with coefficients of relative risk aversion less than  $-5$ . This behavior might represent some form of altruism, as subjects may simply view contributing as a pro-social act. However, such motivations would be misguided, since the group project is a bad bet for other players as well. Furthermore, this explanation is less appealing in AT, in which allocating tokens to the group project may prevent it from being provided.

Another possibility is that subjects are simply bad at calculating expected values. To investigate this possibility, a surprise bonus question was added at the end of the second session of PVT and the second session of AT. In this question, subjects were asked to calculate the expected value of the group project given a signal of 100. Answers within plus or minus 5 of the correct answer (60) were rewarded with a \$1 bonus payment on top of any earnings from the main part of the experiment. If subjects can correctly perform this calculation, they should see that contributing at a cost of 65 is never optimal.

Of the forty subjects in these two sessions, 45% got the answer exactly right (which was also the modal response), and 65% answered within plus

Variable	OR	p-value
signal	1.058	0.001***
period	0.923	0.086*
AT	1.756	0.838
signal*AT	0.950	0.008***
period*AT	1.129	0.097*
correct	0.575	0.423
GPA	0.902	0.837
quant	0.596	0.443
correct*AT	2.648	0.359
GPA*AT	1.227	0.777
quant*AT	0.934	0.946

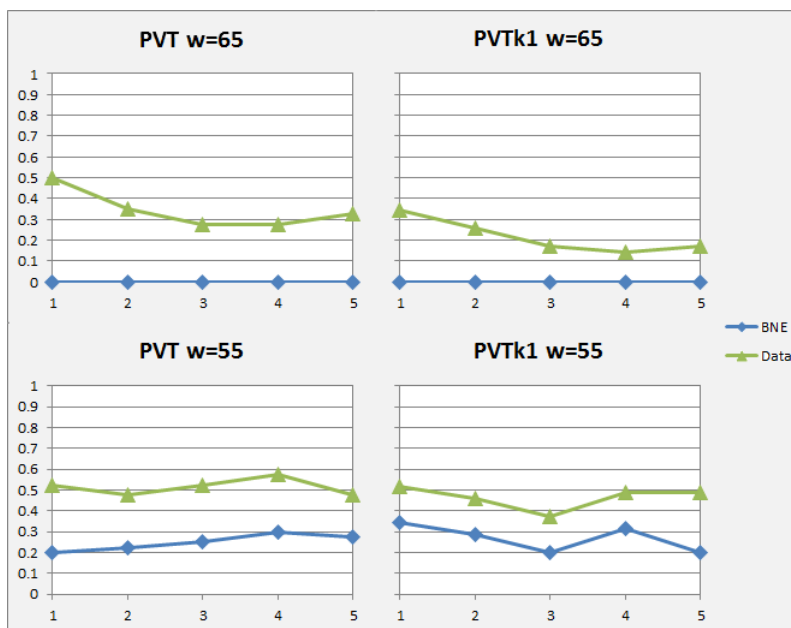
Linear Comb	OR	p-value
correct*(1+AT)	1.524	0.602
GPA*(1+AT)	1.106	0.845
quant*(1+AT)	0.557	0.441

**Table V.** Logistic regression results for bonus question, with contribution indicator at cost level 65 as the dependent variable. Robust standard errors are clustered at the individual subject level. There are 200 observations and 40 subject-level clusters. \*\*\*, \*\*, and \* indicate significance at the 1%, 5%, and 10% levels, respectively.

or minus 5. Table V shows logistic regression results for contribution at cost level  $w = 65$  in these two sessions, using an indicator (“correct”) for an exactly correct answer as an explanatory variable. Answering the bonus question correctly does not appear to be correlated with over-contributing in either PVT or AT.<sup>19</sup> Similarly, over-contribution does not appear to be correlated with GPA or quantitative major. Thus, errors in expected value calculation do not appear to be an important reason for the observed over-contribution.

Another possible explanation is that incentives are weak in PVT with  $w = 65$ , since the probability of provision of the group project is small in this case. To investigate this explanation, I ran two additional sessions of a modified

<sup>19</sup>These negative results are robust to alternative specifications, including using an indicator for an answer with plus or minus 5, or using the actual reported expected value, as well as dropping GPA and quant.



**Figure IX.** *Contribution in PVT v. PVTk1*

version of PVT with  $k = 1$ , which I call PVTk1.<sup>20</sup> This treatment reduces PVT from a game to an individual choice problem, since with a threshold of 1 contributor, no player's choice can affect any other player's payoff.<sup>21</sup> Changing  $k$  does not alter the optimal strategies in this case: the predicted cutoffs are the same as the BNE cutoffs in the PVT game and the fully-cursed cutoffs in the CVT game. However, turning the PVT game into an individual choice problem steepens incentives, since every player is always pivotal in this case.

Figure IX compares contribution in PVT and PVTk1 for contribution costs of 55 and 65. Contribution is significantly lower in PVTk1 than in PVT for  $w = 65$ , though over-contribution is not eliminated.<sup>22</sup> There is no apparent difference for  $w = 55$  or lower costs of contribution (not shown).

<sup>20</sup>The first session of PVTk1 had only 15 subjects due to absences, while the second session had 20 subjects.

<sup>21</sup>Subjects were still matched into groups of five and given feedback on other group members choices, to keep framing and potential imitation learning effects constant.

<sup>22</sup>A Wilcoxon-Mann-Whitney test using the subject-level average contribution as the unit observation shows this difference to be significant with  $p$ -value=0.0228.

Overall, the results in this subsection suggest that over-contribution in PVT with  $w = 65$  are partially driven by weak incentives due to the low probability of provision. Mistakes in expected value calculation do not appear to be a significant factor. Even in the individual choice problem of PVTk1, subjects still choose the group project too often for  $w = 65$ . It is possible that some subjects simply enjoy gambling in small amounts, or use idiosyncratic heuristics leading to over-contribution in this case.

## VI DISCUSSION

In this paper, I have demonstrated that a severe neglect of belief conditioning can lead to under-contribution and thus under-provision of common-value excludable public goods. In the CVT game, a favorable conditioning effect arises in Bayesian Nash equilibrium because the expected value of the public good conditional on sufficiently many others contributing is higher than this expectation conditional on the private signal alone. However, experimental subjects fail to account for this effect, consistent with the cursed equilibrium model of Eyster and Rabin (2005). Furthermore, behavior in this game is indistinguishable from behavior in the closely-related PVT game, in which conditioning effects are absent. There is also a surprising similarity in behavior between the CVT game (with a favorable conditioning effect) and the AT game (with an unfavorable conditioning effect). The fully-cursed equilibria of CVT, PVT, and AT are identical, while there are sharp differences in their Bayesian Nash equilibria. Thus, the similarity in behavior between games is consistent with cursedness. Furthermore, there is little evidence of learning across multiple rounds of play with feedback in any of the three games, and neither cognitive nor quantitative ability appears to mitigate the failure to properly condition beliefs.

However, the level of contribution in PVT with the highest contribution cost is unexplained by cursed equilibrium or BNE. The decrease in contribution in PVTk1 (the individual-choice version of PVT) suggests that flat incentives partially drive contribution in this case, since the probability of provision is low. Such flatness of incentives is also present in CVT with the highest contribution cost, and may have also driven some contributions in this case. If



so, the degree of cursedness in CVT may actually be underestimated, since higher degrees of cursedness lead to less contribution.

While contribution for the highest cost level is significantly lower in PVTk1 than in PVT, it is not eliminated. One possible conjecture to explain this behavior is that some subjects may simply enjoy gambling in small amounts. This conjecture is also consistent with the asymmetry in behavior for cost levels 35 and 65, with over-contribution at cost level 65 but no substantial under-contribution at cost level 35.

This paper contributes to the literature on public goods by identifying a novel source of under-contribution distinct from free-riding. While this experiment is designed to provide a clear separation between equilibrium and naïve contribution choices and not to closely parallel any particular real-world setting, the behavioral phenomenon found here may also be important in more realistic environments. In a number of applications within public economics and industrial organization, such as the provision of gated communities and the formation of joint ventures, naïve contribution choices may cause a failure to coordinate on efficiency-enhancing outcomes. Future research might examine the design of optimal mechanisms for information aggregation in such environments. In the simple case that I consider, the incentives of individual agents are aligned under pure common value, so that agents would truthfully reveal their signals if they could. However, this is not necessarily true in closely related cases in which some form of free-riding is possible. Private value components, unequal contributions, or lack of excludability all lead to the possibility of free-riding in some form, which may give individual players an incentive to misrepresent their private information.

This paper also contributes to the literature on cursedness in related contexts such as common-value auctions and voting games by examining cursed equilibrium in a novel game and showing a potentially important consequence of this type of bounded rationality. Importantly, my experimental design demonstrates the failure to properly condition beliefs by the comparison of the CVT and PVT treatments. While comparing behavior to theoretical benchmarks within a treatment is also useful, the treatment comparison controls for other potential sources of decision error while varying only the presence of

belief conditioning effects. The treatment comparison suggests that subjects not only fail to fully condition beliefs, but actually fail to condition *at all*.

I have focused on the case of excludable public goods (such as gated communities and private parks) to isolate under-contribution due to the neglect of belief conditioning in the absence of free-riding incentives. Future research might explore the idea of pure public goods with interdependent values.<sup>23</sup> Examples include pollution abatement and flood control, for which values are likely to be strongly correlated, but uncertain. This study provides a first step toward a promising line of inquiry on coordination and information aggregation in environments with common-value public goods.

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<sup>23</sup>The neglect of belief conditioning in a pure public goods context might be called a “Free-Rider’s Curse,” though in the current excludable context, there is no free-riding.

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## APPENDIX A: PROOFS

*Proof of Lemma 1.* In symmetric BNE, agents contribute with equal probability  $p = \Pr(c(X) = 1)$ . Given a signal  $x_i$ , agent  $i$ 's expected payoff of contributing is given by:

$$U_i(x_i) = \sum_{l=k-1}^{n-1} \binom{n-1}{l} p^l (1-p)^{n-1-l} \left( \alpha x_i + \frac{(1-\alpha)l}{n-1} E[X|c(X)=1] + \frac{(1-\alpha)(n-1-l)}{n-1} E[X|c(X)=0] - w \right) \quad (10)$$

Differentiating with respect to the  $x_i$ , it is straightforward to verify that agent  $i$ 's expected payoff is non-decreasing in  $x_i$ , and is strictly increasing whenever  $p > 0$ .

Suppose in symmetric BNE,  $\{x_i \in [\underline{x}, \bar{x}] \mid U_i(x_i) > 0\} = \emptyset$ . Then simply let  $x_i^* = \bar{x} + 1$ . Next, suppose in symmetric BNE,  $\{x_i \in [\underline{x}, \bar{x}] \mid U_i(x_i) > 0\} = [\underline{x}, \bar{x}]$ . Then let  $x_i^* = \underline{x} - 1$ . Now suppose in symmetric BNE,  $\{x_i \in [\underline{x}, \bar{x}] \mid U_i(x_i) > 0\}$  is a non-empty, proper subset of  $[\underline{x}, \bar{x}]$ . Take  $x_i \in \{x_i \in [\underline{x}, \bar{x}] \mid U_i(x_i) > 0\}$ . Then since the expected payoff of contributing is non-decreasing in the signal, whenever  $x'_i > x_i$ , it must be that  $U_i(x'_i) \geq U_i(x_i) > 0$ . Since  $\{x_i \in [\underline{x}, \bar{x}] \mid U_i(x_i) > 0\}$  is bounded below by  $\underline{x}$ , it has an infimum. Let  $x_i^* = \inf \{x_i \in [\underline{x}, \bar{x}] \mid U_i(x_i) > 0\}$ . By continuity,  $U_i(x_i^*) = 0$ . By symmetry  $x_i^* = x^*$  for each agent  $i \in N$ .

Given a signal  $x''_i \leq x^*$ , it must be that  $U_i(x''_i) \leq 0$ , by definition of  $x^*$  and continuity of  $U_i$ .  $\square$

*Proof of Lemma 2.* Take  $r \neq i$ . Then:

$$\begin{aligned} & E \left[ X_r \left| \sum_{j \neq i} c^*(X_j) \geq k-1 \right. \right] \\ &= \Pr \left( \sum_{j \neq i} c^*(X_j) - c^*(X_r) < k-1 \left| \sum_{j \neq i} c^*(X_j) \geq k-1 \right. \right) E[X_r | X_r \geq x^*] \quad (11) \\ &+ \Pr \left( \sum_{j \neq i} c^*(X_j) - c^*(X_r) \geq k-1 \left| \sum_{j \neq i} c^*(X_j) \geq k-1 \right. \right) E[X_r] \end{aligned}$$

Intuitively, it is possible to partition the event where at least  $k-1$  signals other than  $x_i$  exceed  $x^*$  into two cases. In the first, exactly  $k-1$  signals other than  $x_i$  exceed  $x^*$ , one of which is  $x_r$ . In the second case, at least  $k-1$  signals

other than  $x_i$  and  $x_r$  exceed  $x^*$ , in which case the expectation of  $X_r$  is simply the prior expectation.

As  $x^*$  increases,  $E[X_r | X_r \geq x^*]$  weakly increases by first-order stochastic dominance. In particular,  $E[X_r | X_r \geq x^*] \geq E[X_r]$ . Since the expression in equation 11 is a convex combination of these two expectations, it suffices to show that the first probability in equation 11 is also non-decreasing in  $x^*$ .

$$\begin{aligned} & \Pr\left(\sum_{j \neq i} c^*(X_j) - c^*(X_r) < k - 1 \mid \sum_{j \neq i} c^*(X_j) \geq k - 1\right) \\ &= \frac{\binom{n-2}{k-2}(1-F(x^*))^{k-1}F(x^*)^{n-k}}{\sum_{l=k-1}^{n-1} \binom{n-1}{l}(1-F(x^*))^l F(x^*)^{n-1-l}} = \frac{\binom{n-2}{k-2}}{\sum_{l=k-1}^{n-1} \binom{n-1}{l} \left(\frac{1-F(x^*)}{F(x^*)}\right)^{l-k+1}} \end{aligned} \quad (12)$$

It is straightforward to verify that  $\frac{1-F(x^*)}{F(x^*)}$  is non-increasing in  $x^*$ , which implies that the probability in equation 12 is non-decreasing in  $x^*$ . Thus the expectation in equation 11 is non-decreasing in  $x^*$ , which implies the result.  $\square$

*Proof of Proposition 1.* Let  $w$  belong to the given interval. First, this interval is non-empty since:

$$\begin{aligned} & \left(\alpha + \frac{(1-\alpha)(k-1)}{n-1}\right)\bar{x} + \frac{(1-\alpha)(n-k)}{n-1}E[X] - \alpha\underline{x} - (1-\alpha)E[X] \\ &= \alpha(\bar{x} - \underline{x}) + \frac{(1-\alpha)(k-1)}{n-1}(\bar{x} - E[X]) > 0 \end{aligned} \quad (13)$$

Note also that the given interval is contained in  $[\underline{x}, \bar{x}]$  since the lower bound is a convex combination of  $\underline{x}$  and  $E[X]$  while the upper bound is a convex combination of  $\bar{x}$  and  $E[X]$ .

Restricting to  $x^* < \bar{x}$ , equation 4 can be rewritten as  $H(x^*) = 0$  where:

$$\begin{aligned}
H(x^*) &= \frac{\sum_{l=k-1}^{n-1} \binom{n-1}{l} (1-F(x^*))^l F(x^*)^{n-1-l} \left( \alpha x^* + \frac{(1-\alpha)l}{n-1} E[X|X \geq x^*] + \frac{(1-\alpha)(n-1-l)}{n-1} E[X|X < x^*] - w \right)}{\sum_{l=k-1}^{n-1} \binom{n-1}{l} (1-F(x^*))^l F(x^*)^{n-1-l}} \\
&= \alpha x^* - w + \frac{\sum_{l=k-1}^{n-1} \binom{n-1}{l} (1-F(x^*))^l F(x^*)^{n-1-l} \left( \frac{(1-\alpha)l}{n-1} E[X|X \geq x^*] + \frac{(1-\alpha)(n-1-l)}{n-1} E[X|X < x^*] \right)}{\sum_{l=k-1}^{n-1} \binom{n-1}{l} (1-F(x^*))^l F(x^*)^{n-1-l}} \\
&= \alpha x^* - w + (1-\alpha)G_i(x^*)
\end{aligned} \tag{14}$$

Intuitively,  $H(x^*)$  is the expected utility of contributing given a signal of  $x^*$  conditional on the public good being provided, where each other agents' strategy is contribution whenever their signal is at least  $x^*$ .

Notice that  $H(x^*)$  is strictly increasing in  $x^*$  because the final term is non-decreasing in  $x^*$  by 2. Thus,  $H(x^*)$  can cross zero at most once, guaranteeing that there is at most one interior equilibrium cutoff.

Consider the behavior of  $H(x^*)$  as  $x^* \rightarrow \bar{x}$ . The key term of interest is the probability that exactly  $k-1$  agents other than  $i$  contributed given that at least  $k-1$  contributed.

$$\begin{aligned}
\lim_{x^* \rightarrow \bar{x}} \Pr \left( \sum_{j \neq i} c(X_j) = k-1 \mid \sum_{j \neq i} c(X_j) \geq k-1 \right) &= \lim_{x^* \rightarrow \bar{x}} \frac{\binom{n-1}{k-1} (1-F(x^*))^{k-1} F(x^*)^{n-k}}{\sum_{l=k-1}^{n-1} \binom{n-1}{l} (1-F(x^*))^l F(x^*)^{n-1-l}} \\
&= \lim_{x^* \rightarrow \bar{x}} \frac{1}{1 + \sum_{l=k}^{n-1} \frac{(k-1)!(n-k)!}{l!(n-1-l)!} \left( \frac{1-F(x^*)}{F(x^*)} \right)^{l-k+1}} = 1
\end{aligned} \tag{15}$$

Since  $\lim_{x^* \rightarrow \bar{x}} \frac{1-F(x^*)}{F(x^*)} = 0$ . Therefore, taking the limit of  $H(x^*)$  as  $x^* \rightarrow \bar{x}$  yields the following:

$$\lim_{x^* \rightarrow \bar{x}} H(x^*) = \alpha \bar{x} - w + \frac{(1-\alpha)(k-1)}{n-1} \bar{x} + \frac{(1-\alpha)(n-k)}{n-1} E[X] \tag{16}$$



Which is positive if and only if  $w < \left(\alpha + \frac{(1-\alpha)(k-1)}{n-1}\right)\bar{x} + \frac{(1-\alpha)(n-k)}{n-1}E[X]$ . Now consider  $x^* = \underline{x}$ .

$$H(\underline{x}) = \alpha\underline{x} - w + (1-\alpha)E[X] \quad (17)$$

The right-hand side is negative if and only if  $w > \alpha\underline{x} + (1-\alpha)E[X]$ . Thus, since  $H(x^*)$  is clearly continuous, there exists  $x^* \in (\underline{x}, \bar{x})$  such that  $H(x^*) = 0$ , thus satisfying equation 4.

Now suppose  $w$  is not within the specified bounds. Since  $H(x^*)$  is strictly increasing in  $x^*$ , it is either positive everywhere or negative everywhere. Thus, no symmetric BNE cutoff exists. □

*Proof of Corollary 1.*  $H(x^*)$  in equation 14 is clearly decreasing in  $w$ . Therefore, when  $w$  increases,  $H$  becomes negative at the previous value of  $x^*$ . Since  $H$  is increasing in  $x^*$ , the value of  $x^*$  at which this function is zero must be higher. Similarly,  $G_i(x^*)$  in equation 2 is clearly increasing in  $k$ , which implies that  $H(x^*)$  is also increasing in  $k$ . Therefore, when  $k$  increases, the value of  $x^*$  at which  $H(x^*) = 0$  must decrease. □

*Proof of Proposition 2.* As in Lemma 1, in symmetric  $\chi$ -cursed equilibrium, the expected utility of contributing is non-decreasing in the signal. Thus by the same argument as in Lemma 1, attention can be restricted to cutoff equilibria. The equilibrium condition in equation 4 becomes:

$$\begin{aligned} & \sum_{l=k-1}^{n-1} \binom{n-1}{l} (1 - F(x_\chi^*))^l F(x_\chi^*)^{n-1-l} \left[ \alpha x_\chi^* + \frac{(1-\alpha)l}{n-1} \left( \chi E[X] + (1-\chi)E[X|X \geq x_\chi^*] \right) \right. \\ & \left. + \frac{(1-\alpha)(n-1-l)}{n-1} \left( \chi E[X] + (1-\chi)E[X|X < x_\chi^*] \right) - w \right] = 0 \end{aligned} \quad (18)$$

As in the proof of Proposition 1, define a function  $H_\chi(x_\chi^*)$  as follows:

$$\begin{aligned}
H_\chi(x_\chi^*) &= \frac{\sum_{l=k-1}^{n-1} \binom{n-1}{l} (1-F(x_\chi^*))^l F(x_\chi^*)^{n-1-l} \left[ \alpha x_\chi^* + \frac{(1-\alpha)l}{n-1} \left( \chi E[X] + (1-\chi) E[X|X \geq x_\chi^*] \right) + \frac{(1-\alpha)(n-1-l)}{n-1} \left( \chi E[X] + (1-\chi) E[X|X < x_\chi^*] \right) - w \right]}{\sum_{l=k-1}^{n-1} \binom{n-1}{l} (1-F(x_\chi^*))^l F(x_\chi^*)^{n-1-l}} \\
&= \alpha x_\chi^* - w + (1-\alpha) \chi E[X] + (1-\alpha)(1-\chi) G_i(x_\chi^*)
\end{aligned} \tag{19}$$

As in Proposition 1, a zero of this function in  $(\underline{x}, \bar{x})$  corresponds to an interior symmetric equilibrium cutoff. As in Proposition 1, it can be shown that  $\lim_{x_\chi^* \rightarrow \bar{x}} H_\chi(x_\chi^*) > 0$  and  $H_\chi(\underline{x}) < 0$  given the bounds on  $w$ . Thus, by continuity, an interior zero exists. By Lemma 2,  $H_\chi(x_\chi^*)$  is strictly increasing in  $x_\chi^*$ , and so it has at most one zero. Furthermore, as in Proposition 1, if  $w$  lies outside the given interval,  $H_\chi(x_\chi^*)$  has no interior zero.

For  $\chi = 1$  (fully-cursed equilibrium), equation 18 becomes:

$$(\alpha x_1^* + (1-\alpha)E[X] - w) \sum_{l=k-1}^{n-1} \binom{n-1}{l} (1-F(x_1^*))^l F(x_1^*)^{n-1-l} = 0 \tag{20}$$

If  $x_1^* < \bar{x}$ , then solving for the cutoff yields equation 7.

□

*Proof of Corollary 2.* From equation 19 it is clear that  $H_\chi$  is non-increasing in  $\chi$ , since by Lemma 2,  $G_i(x_\chi^*) \geq G_i(\underline{x}) = E[X]$ . Thus, as  $\chi$  increases, the zero of  $H_\chi$  (weakly) increases. The proof of the comparative statics with respect to  $w$  and  $k$  is the same as the proof of 1

□

*Proof of Corollary 3.* The bounds on  $w$  in Propositions 1 and 2 imply the result, as long as:

$$\begin{aligned}
&\left( \alpha + \frac{(1-\alpha)(k-1)}{n-1} \right) \bar{x} + \frac{(1-\alpha)(n-k)}{n-1} E[X] - \left( \alpha + \frac{(1-\chi)(1-\alpha)(k-1)}{n-1} \right) \bar{x} - \left( \frac{\chi(1-\alpha)(k-1)}{n-1} + \frac{(1-\alpha)(n-k)}{n-1} \right) E[X] \\
&= \frac{\chi(1-\alpha)(k-1)}{n-1} (\bar{x} - E[X]) > 0
\end{aligned} \tag{21}$$

Which holds if  $\alpha < 1$ .

□

## APPENDIX B: EXPERIMENTAL INSTRUCTIONS

*Experiment Overview*

This is an experiment in the economics of decision-making. In this experiment you will make a series of choices, each of which may earn you money. The amount of money you earn will depend on the decisions you make and on the decisions of others. If you listen carefully and make good decisions, you could earn a considerable amount of money that will be paid to you in cash at the end of the experiment.

*Ground Rules*

Please make all decisions independently; do not communicate with others (in the room or outside the room) in any way during the experiment. This means no talking, no cell phone usage, no texting, no internet chatting, etc. Please do not attempt to use any software other than the experiment software provided. Failure to comply with these rules will lead to dismissal from the experiment.

*Instructions (CVT)*

During the experiment, participants earn tokens. All participants will be paid based on the number of tokens they earn. Each token is worth 20 cents, or \$1 for every 5 tokens.

The experiment consists of twenty rounds. At the start of the first round, you will be randomly and anonymously matched into groups of five. At the start of each later round, you will be randomly and anonymously re-matched into new groups of five, so that your group changes every round and you never learn the identities of the other group members in any round.

In each round, you will choose how to allocate some number of tokens, which we will call  $T$ . Everyone in your group in any given round will allocate the same number of tokens, so that  $T$  is the same for everyone in your group and is known to everyone in your group. However,  $T$  may change from round to round.

The  $T$  tokens may be allocated to a private account or to a group project. If you choose to allocate  $T$  tokens to a private account, then you get  $T$  tokens for the round. The details of the group project are as follows.

In each round, a random number will be selected by the computer from a uniform distribution between 0 and 100. We will call this random number your signal. Each other member of your group will also get a signal randomly selected by the computer from the same distribution. We will call the signals of the five group members  $S1$ ,  $S2$ ,  $S3$ ,  $S4$ , and  $S5$ . All signals are drawn independently. During each round, you will not observe the signals of the other members. Similarly, other members of the group will not observe any signal other than their own.

If you choose to allocate  $T$  tokens to the group project, and if at least three other members of your group also choose to allocate  $T$  tokens to the group project, then you get a number of tokens equal to the average of the signals of all five members of your group for the round. We will call the average of the signals of the five members of your group the value of the group project, or  $V$ , which is given by:

$$V = \frac{S1 + S2 + S3 + S4 + S5}{5}$$

So, for example, if your signal is 50 and the other members of your group get signals of 25, 40, 62, and 86, then the average of all five signals is:

$$V = \frac{50 + 25 + 40 + 62 + 86}{5} = 52.6$$

Thus, in this case, if you chose to allocate  $T$  tokens to the group project and at least three other members of your group also chose to allocate  $T$  tokens to the group project, then you would get 52.6 tokens for that round.

If you choose to allocate  $T$  tokens to the group project, but *less* than three other members of your group also choose to allocate  $T$  tokens to the group project, then you get  $T$  tokens for that round. In other words, if less than four of the five members of your group (including yourself) choose to allocate  $T$  tokens to the group project, then all tokens are automatically reallocated to private accounts and everyone in your group gets  $T$  tokens.

After all members of your group have made their choice, you will learn the value of the group project ( $V$ ) and your earnings in tokens for the round. You will also learn the signals observed by the other members of your group (listed from highest to lowest) and how they allocated  $T$  tokens (to the group project or private account).

The following summarizes your choice in each round. If you choose to allocate  $T$  tokens to a private account, then you get  $T$  tokens. If you choose to allocate  $T$  tokens to the group project, and if at least three other members of your group also choose to allocate  $T$  tokens to the group project, then you get  $V$  tokens, where  $V$  is the average of the signals of the five members of your group. If you choose to allocate  $T$  tokens to the group project, but *less* than three other members of your group also choose to allocate  $T$  tokens to the group project, then you get  $T$  tokens.

Remember that you will be randomly and anonymously re-matched into new groups of five at the start of each round. Also remember that  $T$  is the same for every member of your group. However, signals are randomly and independently drawn for each member of your group.

Of the twenty rounds, one will be randomly selected for payment. All participants will be paid their earnings in dollars for the randomly selected round, plus a \$5.00 show-up payment. You will not find out which round you will be paid for until the end of the experiment, so you should treat each round as something for which you might get paid. You will not be paid for the rounds that are not randomly selected for payment.

Are there any questions before we begin the experiment?

### *Instructions (AT)*

During the experiment, participants earn tokens. All participants will be paid based on the number of tokens they earn. Each token is worth 20 cents, or \$1 for every 5 tokens.

The experiment consists of twenty rounds. At the start of the first round, you will be randomly and anonymously matched into groups of five. At the start of each later round, you will be randomly and anonymously re-matched

into new groups of five, so that your group changes every round and you never learn the identities of the other group members in any round.

In each round, you will choose how to allocate some number of tokens, which we will call  $T$ . Everyone in your group in any given round will allocate the same number of tokens, so that  $T$  is the same for everyone in your group and is known to everyone in your group. However,  $T$  may change from round to round.

The  $T$  tokens may be allocated to a private account or to a group project. If you choose to allocate  $T$  tokens to a private account, then you get  $T$  tokens for the round. The details of the group project are as follows.

In each round, a random number will be selected by the computer from a uniform distribution between 0 and 100. We will call this random number your signal. Each other member of your group will also get a signal randomly selected by the computer from the same distribution. We will call the signals of the five group members  $S1$ ,  $S2$ ,  $S3$ ,  $S4$ , and  $S5$ . All signals are drawn independently. During each round, you will not observe the signals of the other members. Similarly, other members of the group will not observe any signal other than their own.

If you choose to allocate  $T$  tokens to the group project, and if no more than one other member of your group also chooses to allocate  $T$  tokens to the group project, then you get a number of tokens equal to the average of the signals of all five members of your group for the round. We will call the average of the signals of the five members of your group the value of the group project, or  $V$ , which is given by:

$$V = \frac{S1 + S2 + S3 + S4 + S5}{5}$$

So, for example, if your signal is 50 and the other members of your group get signals of 25, 40, 62, and 86, then the average of all five signals is:

$$V = \frac{50 + 25 + 40 + 62 + 86}{5} = 52.6$$

Thus, in this case, if you chose to allocate  $T$  tokens to the group project and if no more than one other member of your group also chose to allocate  $T$  tokens to the group project, then you would get 52.6 tokens for that round.

If you choose to allocate  $T$  tokens to the group project, but *more* than one other member of your group also chooses to allocate  $T$  tokens to the group project, then you get  $T$  tokens for that round. In other words, if more than two of the five members of your group (including yourself) choose to allocate  $T$  tokens to the group project, then all tokens are automatically reallocated to private accounts and everyone in your group gets  $T$  tokens.

After all members of your group have made their choice, you will learn the value of the group project ( $V$ ) and your earnings in tokens for the round. You will also learn the signals observed by the other members of your group (listed from highest to lowest) and how they allocated  $T$  tokens (to the group project or private account).

The following summarizes your choice in each round. If you choose to allocate  $T$  tokens to a private account, then you get  $T$  tokens. If you choose to allocate  $T$  tokens to the group project, and if no more than one other member of your group also chooses to allocate  $T$  tokens to the group project, then you get  $V$  tokens, where  $V$  is the average of the signals of the five members of your group. If you choose to allocate  $T$  tokens to the group project, but *more* than one other member of your group also chooses to allocate  $T$  tokens to the group project, then you get  $T$  tokens.

Remember that you will be randomly and anonymously re-matched into new groups of five at the start of each round. Also remember that  $T$  is the same for every member of your group. However, signals are randomly and independently drawn for each member of your group.

Of the twenty rounds, one will be randomly selected for payment. All participants will be paid their earnings in dollars for the randomly selected round, plus a \$5.00 show-up payment. You will not find out which round you will be paid for until the end of the experiment, so you should treat each round as something for which you might get paid. You will not be paid for the rounds that are not randomly selected for payment.

Are there any questions before we begin the experiment?

*Instructions (PVT)*

During the experiment, participants earn tokens. All participants will be paid based on the number of tokens they earn. Each token is worth 20 cents, or \$1 for every 5 tokens.

The experiment consists of twenty rounds. At the start of the first round, you will be randomly and anonymously matched into groups of five. At the start of each later round, you will be randomly and anonymously re-matched into new groups of five, so that your group changes every round and you never learn the identities of the other group members in any round.

In each round, you will choose how to allocate some number of tokens, which we will call  $T$ . Everyone in your group in any given round will allocate the same number of tokens, so that  $T$  is the same for everyone in your group and is known to everyone in your group. However,  $T$  may change from round to round.

The  $T$  tokens may be allocated to a private account or to a group project. If you choose to allocate  $T$  tokens to a private account, then you get  $T$  tokens for the round. The details of the group project are as follows.

In each round, a random number will be selected by the computer from a uniform distribution between 0 and 100. We will call this random number your signal and label it  $S$ . Each other member of your group will also get a signal randomly selected by the computer from the same distribution. All signals are drawn independently. During each round, you will not observe the signals of the other members. Similarly, other members of the group will not observe any signal other than their own.

Furthermore, in each round, four unobserved random number will be selected for you by the computer from a uniform distribution between 0 and 100. We will label these four random numbers  $R1$ ,  $R2$ ,  $R3$ , and  $R4$ . For each other member of your group, there will also be four unobserved numbers randomly selected by the computer from the same distribution. All of these random numbers are drawn independently of each other and independently of your signal and the signals of others in your group. You will not observe any of these random numbers, and neither will any other member of your group.



If you choose to allocate  $T$  tokens to the group project, and if at least three other members of your group also choose to allocate  $T$  tokens to the group project, then you get a number of tokens equal to the average of your signal and the four unobserved random numbers selected for you by the computer for the round. We will call this average your value for the group project, or  $V$ , which is given by:

$$V = \frac{S + R1 + R2 + R3 + R4}{5}$$

So, for example, if your signal is 50 and the four unobserved random numbers are 25, 40, 62, and 86, then the average is:

$$V = \frac{50 + 25 + 40 + 62 + 86}{5} = 52.6$$

Thus, in this case, if you chose to allocate  $T$  tokens to the group project and at least three other members of your group also chose to allocate  $T$  tokens to the group project, then you would get 52.6 tokens for that round.

If you choose to allocate  $T$  tokens to the group project, but *less* than three other members of your group also choose to allocate  $T$  tokens to the group project, then you get  $T$  tokens for that round. In other words, if less than four of the five members of your group (including yourself) choose to allocate  $T$  tokens to the group project, then all tokens are automatically reallocated to private accounts and everyone in your group gets  $T$  tokens.

After all members of your group have made their choice, you will learn your value for the group project ( $V$ ) and your earnings in tokens for the round. You will also learn the signals observed by the other members of your group (listed from highest to lowest) and how they allocated  $T$  tokens (to the group project or private account).

The following summarizes your choice in each round. If you choose to allocate  $T$  tokens to a private account, then you get  $T$  tokens. If you choose to allocate  $T$  tokens to the group project, and if at least three other members of your group also choose to allocate  $T$  tokens to the group project, then you get  $V$  tokens, where  $V$  is the average of your signal,  $R1$ ,  $R2$ ,  $R3$ , and  $R4$ . If you choose to allocate  $T$  tokens to the group project, but *less* than three other

members of your group also choose to allocate  $T$  tokens to the group project, then you get  $T$  tokens.

Remember that you will be randomly and anonymously re-matched into new groups of five at the start of each round. Also remember that  $T$  is the same for every member of your group. However, signals and unobserved random numbers are randomly and independently drawn for each member of your group.

Of the twenty rounds, one will be randomly selected for payment. All participants will be paid their earnings in dollars for the randomly selected round, plus a \$5.00 show-up payment. You will not find out which round you will be paid for until the end of the experiment, so you should treat each round as something for which you might get paid. You will not be paid for the rounds that are not randomly selected for payment.

Are there any questions before we begin the experiment?

### *Instructions (PVTk1)*

During the experiment, participants earn tokens. All participants will be paid based on the number of tokens they earn. Each token is worth 20 cents, or \$1 for every 5 tokens.

The experiment consists of twenty rounds. At the start of the first round, you will be randomly and anonymously matched into groups of five. At the start of each later round, you will be randomly and anonymously re-matched into new groups of five, so that your group changes every round and you never learn the identities of the other group members in any round.

In each round, you will choose how to allocate some number of tokens, which we will call  $T$ . Everyone in your group in any given round will allocate the same number of tokens, so that  $T$  is the same for everyone in your group and is known to everyone in your group. However,  $T$  may change from round to round.

The  $T$  tokens may be allocated to a private account or to a group project. If you choose to allocate  $T$  tokens to a private account, then you get  $T$  tokens for the round. The details of the group project are as follows.

In each round, a random number will be selected by the computer from a uniform distribution between 0 and 100. We will call this random number your signal and label it  $S$ . Each other member of your group will also get a signal randomly selected by the computer from the same distribution. All signals are drawn independently. During each round, you will not observe the signals of the other members. Similarly, other members of the group will not observe any signal other than their own.

Furthermore, in each round, four unobserved random numbers will be selected for you by the computer from a uniform distribution between 0 and 100. We will label these four random numbers  $R1$ ,  $R2$ ,  $R3$ , and  $R4$ . For each other member of your group, there will also be four unobserved numbers randomly selected by the computer from the same distribution. All of these random numbers are drawn independently of each other and independently of your signal and the signals of others in your group. You will not observe any of these random numbers, and neither will any other member of your group.

If you choose to allocate  $T$  tokens to the group project, then you get a number of tokens equal to the average of your signal and the four unobserved random numbers selected for you by the computer for the round. We will call this average your value for the group project, or  $V$ , which is given by:

$$V = \frac{S + R1 + R2 + R3 + R4}{5}$$

So, for example, if your signal is 50 and the four unobserved random numbers are 25, 40, 62, and 86, then the average is:

$$V = \frac{50 + 25 + 40 + 62 + 86}{5} = 52.6$$

Thus, in this case, if you chose to allocate  $T$  tokens to the group project, then you would get 52.6 tokens for that round.

After all members of your group have made their choice, you will learn your value for the group project ( $V$ ) and your earnings in tokens for the round. You will also learn the signals observed by the other members of your group (listed from highest to lowest) and how they allocated  $T$  tokens (to the group project or private account).

The following summarizes your choice in each round. If you choose to allocate  $T$  tokens to a private account, then you get  $T$  tokens. If you choose to allocate  $T$  tokens to the group project, then you get  $V$  tokens, where  $V$  is the average of your signal,  $R_1$ ,  $R_2$ ,  $R_3$ , and  $R_4$ .

Remember that you will be randomly and anonymously re-matched into new groups of five at the start of each round. Also remember that  $T$  is the same for every member of your group. However, signals and unobserved random numbers are randomly and independently drawn for each member of your group.

Of the twenty rounds, one will be randomly selected for payment. All participants will be paid their earnings in dollars for the randomly selected round, plus a \$5.00 show-up payment. You will not find out which round you will be paid for until the end of the experiment, so you should treat each round as something for which you might get paid. You will not be paid for the rounds that are not randomly selected for payment.

Are there any questions before we begin the experiment?